

Compliments.

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ON THE RELATIONS OF SMALL-POX AND COW-POX,
ESPECIALLY AS ILLUSTRATED BY THE EXPERIMENTS OF
MR. BADCOCK, FORMERLY OF BRIGHTON.*

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I HAVE been induced to bring this subject before your notice on two accounts: firstly, because I believe that Mr. Badcock's experiments, although the most extensive, and consequently the most valuable, of the kind, that have ever been practised, have never adequately been made public; and, secondly, because (perhaps as a consequence of such want of publicity) there have recently been made earnest attempts to propagate a general belief in another set of experiments, which have been interpreted by their conductors to prove the exact reverse of what Mr. Badcock's proceedings do.

I may here state that, to my mind, Mr. Badcock's investigations go to prove the genuineness and validity of the deductions drawn from the experiments of Gassner (1801), of Thiele of Kasan (1839), and of Ceely of Aylesbury (about the same date), to the effect that cow-pox is small-pox modified by its passage through the cow; although, on the contrary, the conclusions of the Lyons Commission (1865), which seem to have been adopted by Italian and Belgian Commissions (the last-mentioned this year), and also strenuously advocated in this country by Veterinary-Surgeon Fleming of the War Office (pamphlet, 1881), would make it appear that small-pox and cow-pox are two entirely distinct diseases, and that the former is so little altered by passing it through the cow, that it will still be infectious small-pox. Which of these two sets of experiments has had a true interpretation given to it by their respective conductors is, of course, of the greatest importance; and my endeavour now will be to satisfy you that the generally received opinion in England—that cow-pox is small-pox rendered mild and uninfectious by being passed through the cow—is that which claims our unqualified confidence and belief.

About forty-five years ago, Mr. Badcock had a sharp attack of small-pox. On recovery, these thoughts occurred to him: Was his previous vaccination defective? Had Jennerian lymph deteriorated? Could a fresh and more vigorous lymph be procured? At length (in 1840), he determined to keep a shed of cows himself, and to experiment upon them.

Although often failing afterwards, it was Mr. Badcock's good fortune

* Read in the Public Medicine Section of the British Medical Association at Ryde, in August 1881.

to have a first success. Obtaining from the late Sir (then Mr.) Cordy Burrows some lymph from a primary case of human small-pox, he inoculated a cow with it, and produced (in the judgment of Dr. Willis and Mr. Burrows) such perfect vaccine vesicles, that he was induced therefrom to vaccinate his own baby, and with the result, again, of satisfying the medical men that genuine vaccine pocks occurred on the little boy's arm. From these, in turn, a supply of lymph was obtained, and was propagated on other children, uniformly with the same results; many other medical men taking part in the proceedings, or using the new lymph in their practices.

Mr. Badcock's enthusiasm carried him forward, and led him, notwithstanding many failures and disappointments, to incur considerable expense in keeping up a stock of cows on which to continue his experiments. For twenty successive years did he persist; from time to time, as opportunity offered, procuring through medical men fresh supplies of variolous matter, and using it on the cows. Altogether, he practised on two hundred cows. By observation, he soon concluded that the udder was not the most eligible part to operate on, the animals often being so impatient and resentful of the use of the lancet there as to interfere with success. Afterwards, taking advantage of the liking which many domestic animals have for their backs to be rubbed, the cowherd would engage the cow's attention by that device (which also would cause her to raise her tail); and thus Mr. Badcock found that he could proceed deliberately to insert his lymph in the thinly haired skin in the neighbourhood of the vulva.

Several of the two hundred animals he inoculated repeatedly. In only thirty-seven instances were satisfactory vaccine pocks considered to result; indeed, in four of these, they were not so finely developed as to encourage a transference from them of their contents. Consequently, the total number of cases in which typical vaccine pocks were produced, and which were considered eligible for vaccinating from (during the twenty years' experiments), was thirty-three. These successful cases were watched, in different stages, by various medical men, many of whom were public or practical vaccinators highly qualified to decide on their merits; and the lymph so obtained has been circulated throughout the civilised world. It continues to be held in high esteem in Brighton, where the three public vaccinators have each of them received gifts from the Government three several times for the high standard of their vaccinations.

The late Mr. Marson used Mr. Badcock's lymph for many years, and so did the late Mr. Ceely. The latter and Mr. Badcock commenced their experiments in the same year, unknown to each other; and Mr. Ceely, having succeeded twice only himself, afterwards visited Brighton, and, having satisfied himself of the thorough reliableness of Mr. Badcock's doings, then ceased personally to experiment further—the more readily so, in consequence of the arduousness of his other occupations as a country hospital surgeon and general practitioner.

This is a sufficient answer to Veterinary Surgeon Fleming's contention (pamphlet, 1881) that Mr. Ceely's experiments are not trustworthy, and could hardly be satisfactory to himself, or he would not have stopped at only two successes.

[I may here point out another unfortunate argument of Mr. Fleming. In trying to show that it was not genuine cow-pox that Mr. Ceely described a Mr. Pollard's five cows and one sturk to have contracted by

grazing in a meadow in which the bedding of small-pox patients was spread, and then to have communicated to their owner's hand, Mr. Fleming omits an all-important part of the narrative—viz., that a man-servant of Mr. Pollard also contracted, from the same animals, pocks on his face and hand, from which Mr. Ceely successfully propagated the disease to children, and carried it on through a great number of successive removes. Plates 3 to 6 inclusive, at the end of Vol. x of the *Transactions of the Provincial Medical and Surgical Association*, 1842, are Mr. Ceely's representations of that young man's pocks.]

To return to Mr. Badcock's doings: I would have you notice that from his thirty-three successfully variolated cows, and from them only, did he ever attempt to convey lymph to human beings. All the other more numerous inoculations, which produced only "minute papules" or other slight irritation, or nothing, he considered as failures. Such minute papules Mr. Ceely also considered as failures in his experiments. These facts have an important bearing on the proceedings of the Lyons Commission, as will be seen presently.

It is a circumstance of priceless value, that the late Mr. Ceely was led to have both his successes and his failures represented by an able artist. Plates xv to xxi in volume viii of the *Transactions of the Provincial Medical and Surgical Association* illustrate the different stages of his eight insertions of variolous matter in one cow, all done at the same time. The four upper he styles "lymphless or abortive tubercles". They seem to be the equivalents of the "minute reddish papules" of the Lyons Commission. Plate xvii represents these as dying away on the eighth day, and the four lower insertions as developed into good pocks. From the latter, lymph was transferred to children with much success, as illustrated by some of the concluding plates in the same volume.

Now, for practical purposes, Mr. Ceely's plates may be considered to illustrate Mr. Badcock's experiments; for, as I said before, these two gentlemen after a while formed an acquaintance with each other, and considered that their respective experiments corroborated each other; after which Mr. Ceely discontinued to experiment.

Mr. Badcock is not a medical man. At the time of commencing his investigations, he was a dispensing chemist. He is now advanced in life, but still retains his wonted shrewd common sense, tempered with much modesty. He says that he never had any literary or scientific pretensions; and hence the little pamphlet, which I now show you, dated 1845, the major portion of which consists of testimonials from numerous medical men, is the only printed record which he has had made of his invaluable experiments; all of which, too, during the whole twenty years, were carried out at his own sole expense.

Sir Thomas Watson and Dr. Bristowe, in their text-books on the practice of medicine, have both accepted Ceely's and Badcock's investigations, as furnishing the true bearing of cow-pox to small-pox; and, as long ago as 1857, in the ponderous "Blue-book" on Vaccination, compiled under the superintendence of Mr. Simon, he thoroughly does the same. These are extracts from his report. "Researches, subsequent to Jenner's, have settled this part of the question. It has been made matter of familiar experiment that the infection of small-pox may be communicated, by inoculation, from man to the cow, producing vesicles which present the physical characters of cow-pox; and that the lymph from these vesicles, if implanted in the skin of the

human subject, produces the ordinary local phenomena of vaccination, from which lymph may be transferred to other unprotected persons, all of which human beings will diffuse no atmospheric infection, and will afterwards themselves cease to be susceptible to small-pox. The people thus protected are so, because the process has really given them small pox of the most mitigated kind. The merit of this discovery belongs in the first place, to Gassner, in 1801; and, of corroborating it in 1840 to Ceely and Badcock. The last-mentioned has, during the last seventeen years,* again and again derived a fresh stock of vaccine lymph from cows thus artificially infected by himself; has vaccinated with such lymph more than fourteen thousand persons, and furnished supplies of it to more than four hundred medical practitioners."

I fancy that now you are wondering why I take such pains to again prove what has been proved and accepted as demonstrated fact so long ago. I stated, at the beginning of this paper, why I do so—viz. : because several influential people, basing their notions on the doings of the Lyons Commission, are not only disputing the character of Ceely and Badcock's doings, but are even daring to pronounce their vaccinations to have been the means of spreading infectious small-pox. Here is the report of the Lyons Commission. At page 56 begins the chapter headed "The local and general effects produced on the ox by the inoculation of human small-pox". Here I read that seventeen animals were variolated—the females on the vulva, the males on the perinæum or scrotum; and some of them by merely subepidermic pricks, others by punctures passing through the true skin. On each animal, no general constitutional effects whatever resulted. The local effects were so little pronounced as totally to escape the notice of the observers at first; but, by the fifth day, there existed at the sites of the superficial punctures, in all the animals, "very small reddish papules", from two to four millimètres in diameter. This was when they had attained their maximum. By the twelfth day they had completely disappeared, never having presented the least vesicular or pustular appearance, nor any crust beyond an extremely small blackish one in the pricks themselves. At the sites of the deeper punctures (some of which had had the variolous lymph passed into them freely by a cannulated needle), less local effects still developed themselves.

Further on, we read that, from these *très-petites papules rougeâtres* (very small reddish pimples), the Commission, by dint of excising and scraping, obtained enough "serosity" on the point of a lancet to inoculate the arm of a child (unvaccinated) with one insertion; and from it, in due time, they inoculated a second child—the result being that both children manifested the local and general symptoms of small-pox, the first child being more covered with the eruption than the second. Can we not find it easy to believe, with Dr. Robert Cory,† that the operator, in his anxiety to get lymph from a "lymphless tubercle", recollected some of the variolous matter which had been placed there a few days previously. What had dried on the surface might be remoistened by the "serosity" obtained by puncturing the papule, and then be

* This was written in 1857; and Mr. Badcock continued his experiments for some years after that.

† "Thesis on the Relation of Cow-pox and Horse-pox to Small-pox," *St. Thomas's Hospital Reports*, 1879.

gathered on the lancet by the "scraping" described as practised. Anyhow, the Commission considered that these two children in the Hôpital de la Charité at Lyons, had had small-pox conveyed to them by their operations; and they were thereby deterred from carrying on their experiments in that manner any further.

Now we can compare and contrast the doings and conclusions of the Commission with those of Gassner, Ceely, Badcock, etc.

The Commission operated on seventeen oxen, and did not, in any one of them, succeed in producing anything like a vaccine vesicle; nevertheless, they considered that they had successfully variolated every animal. It appears that, unfortunately, they had not made themselves acquainted with the details of Ceely's doings, and especially had not seen his plates; moreover, they seem never even to have heard of the more extensive experiments of Badcock. The "minute reddish papules" on the cows, which the Commission thought successes, Ceely and Badcock considered failures; and the (typical) successes of the latter occurred but seldom: with Ceely, only twice in I know not how many trials; with Badcock only thirty-seven times in more than two hundred trials; with Gassner, once after ten failures; and similarly with most other attempts in this country.

The Lyons Commission had water-colour drawings made of the appearances in their experiments, and advertised an atlas of plates to be copied from them. This atlas, however, I am informed by the intended publisher, was never issued. The Commission consisted of nine eminent men; and, during their two years of office, they conducted a great variety of experiments bearing on vaccination—a large number of oxen, horses, donkeys, etc., being placed at their disposal, as also the resources of the Veterinary College. It is, therefore, not surprising that their conclusions are accepted and approved by most pathologists on the continent and even by a few eminent men in this country, who are insufficiently informed concerning the researches of Ceely and Badcock. One of our intended visitors here (Dr. Warlomont of Brussels) has kindly presented me with a copy of his Report on Vaccination, as rendered to the Belgian State this year; and in this I see that he adopts the views of M. Chauveau and his fellow-labourers.

In conclusion, I have to thank you, gentlemen, for your patience in listening to my discursive account of this subject. I wish the performance had been undertaken by somebody else, who would have done it more ably, and with the full justice which I believe it deserves—thereby raising to a becoming height of respect in the minds of his countrymen the vaccination experiments of John Badcock; but, *Magna est veritas et prævalebit*.

P.S.—Since the above was read at Ryde, M. Pasteur's remarkable communication on Chicken Cholera and Splenic Fever, as communicated to the International Medical Congress in London, has appeared in the journals.

The resemblance between M. Pasteur's experiments and those of M. Thielé (by which he declares he artificially reduced small-pox to cow-pox without passing the small-pox through the cow) is so striking, that I here briefly append M. Thielé's account of the process.

He says: "Take some human small-pox lymph, and keep it between waxed glasses for ten days; then moisten and dilute it with cow's milk, and with this inoculate a child. The lymph from this child is again to

be kept between waxed glasses for ten days, then diluted with milk, and transferred to another child ; and so on—the same process being repeated to a tenth child. By this time, the disease will have become as benign and non-infectious as cow-pox, the successive ten children having manifested it in a gradually milder form, the secondary fever and the secondary pustules around the inoculated part having gradually ceased to occur ; and thenceforth the lymph may be propagated directly from child to child (without keeping or milk-dilution) just as in ordinary vaccination."

The above was published in 1839, in *Henke's Zeitschrift für die Staatsarzneikunde*; and is quoted by Mr. Simon in the Blue-book on Vaccination in 1857. How closely analogous is it to M. Pasteur's plan of keeping, and diluting, over and over again, with chicken-broth, a drop of the blood of a fowl dying of chicken-cholera, and then using it, by inoculation, to produce a mild substitute for the natural deadly disease !
